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SOME PROBLEMS OF LOGICAL METHOD IN POLITICAL ECONOMY

The development of statistical data and technique, and the marked tendency toward the broadening of the scope of economic inquiry, have again made the problem of economic method an appropriate theme for discussion. This discussion cannot be left entirely to the logician, partly because he shows little interest in it and partly because there are many phases of it on which the economist can best throw light. There is a tendency among economists to fear overmuch for the integrity of their science and to try to maintain its borders intact by carefully avoiding encroachment on the fields of other sciences. Specialization has its advantages, as the economist has good occasion to know, but unorganized specialization means confusion instead of co-operation. If any body of scientists fail to adapt the subject-matter of their particular science to the needs of other sciences, each of the other sciences must appropriate from it such of its data as it needs for its own purposes, and even, if necessary, retain for itself the right of judgment on disputes within that science which have bearing on its own problems. And in no other of its border-line problems does political economy so urgently require a recasting and reanalysis of its principles as in the problems of logical method in political economy.

The logical doctrines of the average economist are antiquated and inadequate for his needs. Furthermore, the literature of economic method is dominated by the writings of a group of economists who were at the same time logicians of a narrow and largely discredited school. These economists, influenced by their general logical dogmas, either rejected induction *in toto* as a possible method of economic research, or gave it only grudging admission as the veriest handmaid to the deductive method. Contemporary logic has thrown new light on the character and possibilities of the inductive method, and has conceded it a much more generous scope.

If the economist follows the old economic logicians in their rejection of induction, his own practice, generally better than his precepts, forces him into contradictions from which he finds only partial escape by belated and inconsistent concessions to a method of whose value he would at first admit nothing. It is the purpose of this paper to set forth the differences between the two methods as they concern the economist, to state and examine the case which has been made against the use of the inductive method in economic research, and to outline the possibilities and the limitations of the method. The discussion of such logical problems and the use of such technical terminology as do not seem absolutely essential to the presentation of the argument will be carefully avoided.¹

Deduction and induction are the two logical processes of thought, and are the reverse of one another in character, the former being the process of analyzing a generalization into its particular propositions, the latter the process of building up a generalization from a number of particular propositions. The deductive inference must start with a generalization, and generalizations are obtainable only in three ways: by complete enumeration of the individual propositions comprising the generalization, by hypothesis or assumption, or by inductive inference from incomplete enumeration. The first method is not a method of reasoning, since it merely supplies us with a collective expression for a number of known identical propositions; the second becomes inductive as soon as an attempt is made to bring it into touch with reality, since the generalization derives what validity it may thus obtain from the particular instances found to support it; the third is of course pure induction. Unless a science is wholly abstract or hypothetical, it must therefore rely on inductive inferences for its fundamental general propositions, and must consist largely of inductive inference and the deductive application of

¹ In my analysis of the various logical methods I adhere in general to the treatises of Sigwart, Laurie, Bradley, Venn, and Mellone. I may have departed in some instances from the conventional treatment of the problems, especially in questions of interpretation, and I do not wish to attribute the responsibility for any of the logical doctrines here stated to any particular writers, unless I make specific reference to them.

such inference to narrower groups of instances. Only the mathematical sciences are purely deductive, and they have remained so by remaining purely abstract.

Inductive reasoning has its psychological basis in our instinctive tendencies to believe in the existence of a uniformity of causation in nature, and to argue from analogy and the association of ideas. In its fully developed scientific form it is of course much more. True induction is never merely the inference, from a few or many observed instances of the coexistence of two phenomena, of the universal necessity of such a relationship. It demands the intuitive belief, obtainable only through knowledge and insight of the phenomena, that the coexistence was a necessary one in each instance of its discovery. It demands, also, the careful examination of new instances, before we may conclude that they resemble the old in the characteristics which seem to be essential to the existence of the supposed relationship. Furthermore, it demands the absence of known contrary instances, and the assurance that such contrary instances would have been discovered did they exist. Only after these conditions have been fulfilled may we be reasonably certain of the truth of our conclusion. Inductive reasoning can never bring absolute certainty, but it may bring any degree of conviction from a mere probability to what may be accepted as certainty for all practical purposes. But the deductive method cannot bring more certain positive results, since its conclusions are either hypothetical or are derived from inductive premises of uncertain truth in the logical sense. The academic logician, whether he be economist or not, who demands absolute certainty as the canon of research, who sees no pause in the descent from absolute certainty to random guesswork, is therefore setting up a standard impossible of attainment in any but the purely abstract sciences.¹

The nature of the methodological problem in political economy is not, therefore, whether induction can or must be entirely dispensed with—unless there is any sound argument in favor of

¹ Cf., e.g., Bertrand Russell, *Principles of Mathematics*, I, 11: "What is called induction appears to me to be either disguised deduction or a mere method of making plausible guesses." Cf. also E. B. Holt, *The Concept of Consciousness*, p. 12, for a similar opinion.

making economics a purely abstract, hypothetical science. Nor need it necessarily be conceived as a comparison of the relative importance of the two methods. The important problem for the economist is rather the question of the possibility of an extended use of the inductive method in research in other than a fashion purely subsidiary to deduction.

The economists have surpassed the logicians, as is perhaps natural, in the looseness with which they have used the term induction. Thus, by Pierson¹ the value of induction in economic research has been contrasted with the value of reasoning, as if the two terms were opposites. By many writers of the "historical school" induction was asserted to be the principal and even the only method of political economy, although they often used the term as if it meant the simple observation of particular phenomena without any attempt at the inference of general principles therefrom.² It is no longer necessary to spend time in refuting the claim that the mere record of the historical sequence of facts can be made to reveal an enlightening philosophy of history or to disclose without further analysis or interpretation an explanation of contemporary economic phenomena. But the identification of the historical with the inductive methods is a mistaken one, and the weakness of the former in nowise involves the latter.³

The cause of induction has been further weakened by the excessive claims made for it and of it. Thus Mayo-Smith's contention that by a simple process of analogy we can reason "from the prosperity of England to the principle of free trade, at least for industrially developed nations,"⁴ is an exaggeration of the ease of research regardless of the methods employed, and is based on an inadequate notion of the nature and limitations of the inductive method, derived largely from his intellectual connections with the "historical school." Similarly the critics of the inductive method have chosen their illustration of its inefficacy from just those problems which are the last to which the inductive economist

¹ *Principles of Economics*, pp. 33-36.

² Cf. Ingram, *History of Political Economy*, New York (1907), p. 204.

³ Cf. *Ibid.*, p. 213, and Bagehot, *Economic Studies*, 2d ed., p. 12.

⁴ *Science Economic Discussion*, p. 114.

would attempt to apply his methods. Mill offers, as tests of the possibility of the use of induction in political economy, such problems as the determination of "the effect of a particular circumstance in education upon the formation of character,"¹ and believes that the failure of the inductive economist to solve such problems demonstrates the futility of his method.

The objections to the use of the inductive method which have proved to be of most weight are those made by that group of English economists who were at the same time logicians of considerable authority, and were thus peculiarly able to impose their logical doctrines on economic literature.² Most important of these was John Stuart Mill, and it is to his criticism of the method that we shall chiefly direct our attention.

Mill defined induction more broadly than do most logicians, and did not demand for its successful application the fulfilment of all those conditions which we have put forth as essential to the true inductive method.³ Furthermore, he interpreted induction as a method bringing conclusions of absolute certainty. As a result of the breadth of his definition and the extent of his demands of the method, he was led to limit the possible application of the method to the fields in which the difficulties were fewest and of least importance. What these alleged limitations are, we cannot grasp until we have examined Mill's exposition of the technique of induction.

¹ *Essays on Some Unsettled Questions of Political Economy*, 3d ed. (London, 1877) p. 147.

² Senior, *Political Economy*; Cairnes, *Character and Logical Method of Political Economy*; J. S. Mill, *System of Logic and Essays on Some Unsettled Questions of Political Economy*; Jevons, *Pure Logic and Principles of Political Economy*. These form an extreme group in their condemnation of the inductive method. Sidgwick, *Principles of Political Economy and Scope and Method of Political Economy* (in collected *Essays*); Keynes, *Scope and Method of Political Economy*; and Bagehot, *Economic Studies and Postulates of Political Economy* (unfinished), are more moderate, but make induction subordinate.

³ "Induction is that operation of the mind by which we infer that what we know to be true in a particular case or cases will be true in all cases which resemble the former in certain assignable respects. In other words, Induction is the process by which we conclude that what is true of certain individuals of a class is true of the whole class."—*System of Logic*, 7th ed., I, 319.

The technique of induction, first developed by Herschel, was given its most elaborate treatment by Mill in his *System of Logic*. His analysis of the methods of induction has been severely criticized by logicians, but the main points have remained intact, and it is mainly on questions of interpretation and formulation that his treatment has received any very important modifications. I shall follow Mill's treatment, except where the revisions suggested by modern logicians are of significance to the problem of economic method.

The purpose of an induction being supposed to be the proof that A is a cause of B, the problem of induction is, What are the possible methods of obtaining this proof? They all resolve themselves into a demonstration that B is present wherever A is present, and that B is absent wherever A is absent. If, in a number of otherwise diverse situations, B is always found to be present when A is present, we may conclude that A and B are causally connected—the method of agreement. If the addition of A to a situation from which it was formerly absent causes B to appear in the situation, and if the subtraction of A from the situation causes B to disappear, we may conclude that A and B are causally connected—the method of difference. Each of these methods may be improved if in each case we examine all the negative instances of A, and find that B is always absent when A is absent—the methods of double agreement and of double difference. These are the primary methods, from which are derived two other methods, the methods of residues and of concomitant variations. These latter Mill seems to think of quite minor importance, and in his discussion of the scope of the inductive method in political economy he practically ignores them. We will reserve further consideration of them until we have dealt with Mill's treatment of the primary methods.¹

¹ In his description of the methods of induction Mill speaks only of the mechanical means of discovery of correlations, and ignores entirely the further problems of hypothesis and of intuitive discovery of the necessity of such correlations as are found in each instance of their occurrence. As has been aptly remarked, Mill "ignores the constitutive faculty of the mind" (W. H. Blunt, article on "Logic," *Encyclopaedia Britannica*).

The method of (single) agreement is applicable where experimentation is not possible. Mill asserts, however, that the method is unreliable for ascertaining causal sequences, because of the so-called plurality of causes to be found in nature. The method of (single) difference, on the other hand, almost always demands experiment, since it is very rarely that one finds in nature several situations resembling each other in every possible respect except the presence in some and the absence in others of a single cause and its effect. The "double methods" both require experiment for their application, since in no other way can we make sure that we have exhausted all the instances in which the cause to be studied is absent. Because of the existence of the plurality of causes, the use of the inductive method as an independent method of research is therefore impossible unless resort can be had to experiment and the artificial manipulation of material. Since the plurality of causes is most prominent in the phenomena with which the social sciences deal, and since experiment is practically impossible in these sciences, Mill is finally led to conclude that the inductive method is of no value in these sciences.¹

The plurality of causes, of which Mill makes so much, is not nearly so important as he supposed. Mill does not distinguish between the doctrine that different causes may produce the same effect, to which alone the term may properly be applied, and the entirely independent doctrine that different causes may combine to produce a joint effect, but gives his adherence to both without any discrimination. Plurality of causation, if it were really existent in nature, would place serious obstacles in the way of scientific research not only by means of the inductive method, but by means of any of the methods of thought with which we are acquainted. But it seems to be the consensus of modern opinion, both among logicians and among the physical scientists, that the plurality of causes is apparent rather than real; that the belief in the plurality of causes is the result, not so much of its actual existence in nature, as of our inability to reduce nature to its simpler elements. The apparent plurality vanishes before exact investigation, wherever such investigation is possible. But the concurrence of causes,

¹ *Some Unsettled Questions*, pp. 137-53; *System of Logic*, Bk. vi, *passim*.

the combination of causes to produce a joint effect, is to be found in all actual situations. The doctrine of plurality, therefore, is only a practical working caution. Until we definitely discover the *immediate* cause, we must remember that what are apparently different causes may bring about the same event. If we can extend our investigation, we shall always discover, however, that the same immediate cause is contained in all these combinations of causes.

The problem of induction is to analyze these combinations of causes and of effects into their separate elements, and then to proceed to the attempt at discovery of necessary relationships between the individual causes and the individual effects. Is isolation of factors for observation and experiment as impossible for the economist as Mill believes it to be? And is isolation of factors the only possible method of analysis of complex economic situations? To answer these questions and to explain Mill's affirmative answers, we must examine his statement of the nature of the subject-matter and the problems of political economy.

Mill conceives political economy to be a psychological science. Political economy is "the science relating to the moral or psychological laws of the production and distribution of wealth."¹ The laws of political economy, according to Mill, are laws of mind. And the methods applied to the investigation of laws of matter cannot be used in the determination of the laws of mind, since "laws of matter and laws of mind are so dissimilar in their nature, that it would be contrary to all principles of rational arrangement to mix them up as part of the same study. In all scientific methods, therefore, they are placed apart."² Human psychology is not available for inductive study, because of the differences between individual minds, the immense multitude of the influencing circumstances, the practical difficulty of experiment upon human beings.

Even in operating upon an individual mind, which is the case affording greatest room for experimenting, we cannot often obtain a crucial experiment.

¹ *Some Unsettled Questions*, p. 133.

² *Ibid.*, p. 130. Cf. Cairnes, *Character and Logical Method of Political Economy*, pp. 43-54, for a critical examination of Mill's doctrine.

The effect for example of a particular circumstance in education, upon the formation of character, may be tried in a variety of cases, but we can hardly ever be certain that any two of those cases differ in all their circumstances except the solitary one of which we wish to estimate the influence. In how much greater a degree must this difficulty exist in the affairs of states.¹

Political economy, therefore, is conceived by Mill as a study of human psychology. But not of all human psychology, or even of real human psychology, but only of an assumed psychology.

Geometry presupposes an arbitrary definition of a line, "that which has length but not breadth." Just in the same manner does Political Economy presuppose an arbitrary definition of man as a being who invariably does that by which he may obtain the greatest amount of necessaries, conveniences and luxuries, with the smallest quantity of labour and physical self-denial with which they can be obtained in the existing state of knowledge. . . . Political Economy, therefore, reasons from *assumed* premises—from premises which might be totally without foundation in fact, and which are not pretended to be universally in accordance with it.²

But the assumptions are not confined to the "economic man." They are extended, although less explicitly, to the environment to which this creature is supposed to react. Private property, perfect competition, *laissez-faire*, the English land-tenure system, are assumed to be as universal as the "economic" psychology.³ A hypothetical psychology and a hypothetical environment cannot, of course, be subjected to anything but hypothetical examination or experimentation. So long as the science is built upon such a basis, Mill is justified in claiming deduction as its only possible logical method, and that not a positive but an "a priori" or hypothetical deduction.⁴

¹ *Some Unsettled Questions*, p. 147.

² *Ibid.*, p. 144.

³ Mill seems to have succeeded in keeping this rather heroic assumption implicit, only because of his prepossession with the psychological aspects of economic data. Some of his followers were pried apart from this attitude by the criticisms of the historical school. Cf. e.g., Bagehot, *Economic Studies* (London, 1888), p. 19 (2d ed.), and Ingram, *History of Political Economy*, p. 223, for a discussion of Bagehot's defection.

⁴ It was the tendency of the English school to reason from hypothetical premises, rather than their use of the deductive method, which was objected to by the "historical school." The "historical school" did not expect much from the use of the inductive method, but demanded that deductions, if they were made at all, should be made from categorical premises obtained from historical material.

It was in this way that the English economists hoped to build an abstract economic science, closely resembling the mathematical sciences, and indeed with these sciences as their model. From the limited number of hypothetical propositions of human psychology and of the social environment accepted as the fundamental postulates of the science,¹ there was to be derived the whole body of economic doctrine. The validity of this doctrine was conditional upon the validity of the postulates upon which it was built. If anyone chose to compare these doctrines with reality, he was at perfect liberty to do so, but in doing so he was stepping beyond the bounds of economic science, and indeed of any science. "To verify the hypothesis itself a posteriori, that is, to examine whether the facts of any actual case are in accordance with it, is no part of the business of science at all, but of the *application* of science."² When the abstract economists concede that the work of testing and verifying the hypotheses, and of bringing them into agreement with reality, is to be done by induction—as they often do—this does not therefore involve the concession of a place for induction in economic research.³

The abstract economists exaggerate the possibility of obtaining a vast deal of knowledge from a system of deductions derived from an initial set of four or five propositions. Even the mathematical sciences, although they have the advantage of a completely abstract set of fundamental propositions, could not advance very far were it not that they start either with an infinite series of such propositions, as does algebra, or, like geometry, are continually introducing

¹ Several attempts have been made to formulate the fundamental postulates from which all economic knowledge was to be derived, and they have been reduced to as few as two. Four seems to be the favorite number, however; cf. Senior, *Political Economy*, p. 26; Cairnes, *Logical Method*, p. 56; Cossa, *Political Economy*, p. 74; and for a discussion of these formulations, Sidgwick, *Principles*, p. 35 (2d ed.), and Keynes, *Scope and Method*, p. 243.

² *Some Unsettled Questions*, p. 143.

³ For a criticism, by an a priori economist, of the view that the study of disturbing causes does not come within the bounds of economic inquiry, see Cossa, *Political Economy* (London, 1893), p. 78. Cossa also differs from Mill in contending that the "perturbing causes" can be most effectively investigated by induction, and thus grants to induction the important task of relating the hypotheses of the abstract political economy to the actual facts, as learned by inductive examination.

new material in the form of postulates, axioms, definitions, and hypotheses. The possibility of drawing out new knowledge in an endless stream from a given set of initial propositions, as a magician draws endless ribands of paper from an empty hat, is a purely fictitious one. And of the two alternatives to the economist, of either relying on his imagination or resorting to a study of economic facts for his new material, the latter method is probably less subject to exhaustion.

The contentions for such heroic abstraction as was advocated by the English economists seem to be as follows: Men are motivated by such conflicting desires, and the possibility of direct observation of these desires in isolated operation is so limited, that only by abstraction from all the motives which actuate men except the "economic" one, and only by deduction from our knowledge of the action of this desire obtained through our introspective examination of its operation within ourselves, and our inference—induction!—that it operates likewise within other individuals, can we get sweeping generalizations of universal application. Similarly, only by assuming a uniform environment, and again relying on our introspection for the discovery of how the "economic" motive would operate in such an environment, can we save our generalizations, which apply to all men, from limitations by differences in the situations in which these men find themselves. To modify or reduce the extension of our generalizations as differences appear in the psychology and environment of different individuals or groups, is to step beyond the bounds of economic science into the field of application of science.

This reasoning does not seem very convincing. Almost always the value of very wide generalizations lies in their being broken up into narrower ones. We have in actual life to deal with special, and not general, situations, and the value of general principles consists in their provision of starting-points from which to derive less general principles, covering fewer instances, but telling us more of each instance. The old knowledge in each such case is obtained from the wider generalization, the new knowledge is the product of an inductive inference. In order to derive any benefit from our knowledge of how the "economic motive" operates in

a hand-picked selection of more or less hypothetical situations, we must compare that knowledge with what additional knowledge we can obtain from an examination of the actual situations in their multiform variety.

How much of the reduction of generalizations, to apply to narrower and more completely described groups of instances, may be left safely to the individual practicing the art built upon that science, how near we may approach to particularism, at what stage the scientist may assume that he has obtained sufficient insight into the situations and their problems to make uneconomical and unwise a still further analysis, only individual practice and experience can reveal. The scientist, developing methods and tools of research, must confine himself to fairly extensive generalizations. The individual, practicing the art derived from that science, must start where the scientist leaves off, and end finally at the handling of particular facts. We have been too much dominated by the reasoning of Mill and his school, to the effect that application begins and political economy ceases as soon as we depart from the romantic generalizations of widest possible scope—generalizations as thin as they are broad. As a result, we are accustomed to regard the fields of money, or of labor, or of commerce, as “applied economics,” lying outside the field of economic theory. Occasionally we even meet a worker in these fields who claims freedom from the necessity of using economic “theory” or any theory. These critics of economic “theory” are, let us hope, really critics of those excessive generalizations of the pure deductive school, which are not always capable of being brought into closer touch with actual phenomena, and which, even if capable of comparison with reality, remain either extremely devoid of content or extremely removed from direct relevance to the problems of this world. But theory, generalization, the systematization and organization of facts and the inference of causal laws therefrom, they must resort to, even in their “applied” fields, or they cease to be scientists, and become either descriptive artists or plyers of trades.

The abstract economists justify their method by appeal to the example set by the mathematical sciences, a case of faulty inductive

inference. For the abstractions of the a priori economists, particularly if these economists conformed in practice to the precepts dictated by their methodological theories, would be extremely heroic, while those of the mathematician are for ordinary purposes of little practical import. The areas contained in points and lines do not trouble the carpenter, unless he is using a blunt pencil; and the relations between ten and twenty which are only postulated by the mathematician for ten in the abstract and twenty in the abstract seem to work equally well when the ten or twenty are pounds or cabbages or dollars.

But the "postulates" of the classical political economy, while restricted and scanty enough, were not as hypothetical or "assumed" as was supposed by the economists who formulated them. The psychology of the "economic man," faulty and unsatisfactory as it was, in the one characteristic essential to the economist above all others was not nearly as remote from reality as his creators supposed. In fact, it may almost be said that the "economic man" was an actual Englishman of the commercial world, the description of whose behavior was correctly obtained by inductive inference from observation, but marred and distorted by faulty deductions from an inaccurate introspective, speculative psychology, in an attempt to obtain a rational explanation of the motivation of his behavior. In his commercial activity, with which the economist is primarily concerned, man is thoroughly economic. As economists we are concerned with his ends and not with his motives. His motives may be numerous enough and complex enough to merit the abstractions of the old economists, but in his ends he is simple enough for inductive investigation. The bottle of medicine for a dying child, or of wine for himself; the tools for his trade; the supplies for a home for the aged, bought as a contribution to the home from a future inmate—all are bought with the same end of getting the most for the least, whatever the motive for the purchase may be. Nor in asserting that the ordinary individual, in his economic activity, of his possible alternatives follows the one he most desires to follow—which is all the economist need assert—do we preclude ourselves from the admission

that a laborer will not necessarily seek the higher wage if it involves the harder work or the longer day.

It is of course allowable in the interests of science to start from such a fiction as that a number of persons are ruled only by egoism, and from the further fiction that the means of satisfying this egoism are seen and applied by them reasonably and consistently. But because a reality corresponding to these hypotheses can be nowhere ascertained, the deductions of this exact process cannot be confronted with reality.

It seems a more fruitful way, and one more easily compared with reality, to start from ends. To obtain the greatest possible value with the least possible outlay of labour and capital is an end, which is not only quite comprehensible, but is extensively present and acknowledged; from what motives individuals place this end before them is a further question which it is not necessary to take into immediate consideration for the consequences of the end. . . . It is this end which must logically determine the behavior of man to a large extent. Whatever follows this end under given actual conditions can be constructed and can be directly compared with reality. . . . In construction from motives we must work from the fictitious normal man.¹

In commerical activity the economic end fails to act in isolation only to the extent to which one party to an economic transaction concerns himself with the motives or welfare of the other. With the growing degree of impersonality this moral factor becomes negligible; and the economic transaction becomes non-moral in the sense that each party excludes the other from his moral situation. The parties may not know each other, may deal only through an intermediary, human or mechanical, and except for the plane of competition as dictated by the various relevant forms of social control neither is influenced by anything but his immediate economic end. The plane of competition is always, at least in commercially advanced countries, a lower one morally than that on which man's more personal contact with man takes place. Commercial transactions were not always impersonal—and therefore non-moral—nor are they completely so in special instances in advanced countries, or generally in backward countries. One cannot argue toward the determination of contract rents paid by tenant farmers in England on the basis of a deductive theory of rent resting on the complete dominance of the economic end—

¹ Sigwart, *Logic* (London, 1895), II, 456-57.

as one may perhaps for urban rents in New York City. But if an inductive examination of the situation studied should lead us to believe that the economic end is dominant there in so far as concerns economic transactions, we are not driven to abstractions to discover what are the reactions of the group of individuals to the various economic phenomena within that situation.

The economist is concerned with human behavior, and with human psychology only in so far as it is necessary for an explanation of human behavior in its economic aspects. In order to learn how men will act in a given situation, or how a change in the situation will modify their behavior, it is surely more practical to observe their behavior than to attempt to discover by introspection or otherwise what they might be supposed to do if actuated by a certain motive operating alone. Both methods are inductive, but the former seems to meet the canons of induction much more satisfactorily than the latter. Even the psychologist as such is beginning to rely less and less upon abstract, speculative propositions, and to explain human psychology by the acts of men under given circumstances rather than by the mental processes behind these acts, which are not subject to inductive examination. The modern trend in psychology is decidedly away from introspection and the attempt to explain behavior by rational motives revealed by introspection. The psychologist looks rather to the systematic observation of behavior as the source of psychological generalizations, and uses the inductive method—experimental only in part—as a means of obtaining his general principles. The part which consciousness plays cannot be so revealed, of course, and here the method of inference from specific observation fails, but the economist as such is only concerned with the external aspects of human psychology, and can well afford to leave the analysis of motives to the speculative psychologist. The bonds which tie political economy to an out-of-date rational hedonistic psychology and its appropriate logical method of investigation are not indissoluble.

But of even more importance to the fixation of economic method than the shortcomings of their psychology is the failure of the English classical economists, in their writings on methodology,

to realize that political economy is not wholly or even predominantly psychological in character. One would be justified in concluding from Mill's definition and description of the science that political economy was a subdivision of psychology, concerned with the workings of one human motive. The economist is not only concerned with man's reactions to his environment in their objective manifestations, but he must also examine and analyze the environment and study the effects upon it of man's reactions to it. Political economy has been too often described as if it were merely a "pure" or a priori psychological theory of value and distribution. Of much greater importance to the economist than any "pure" theory is the knowledge and understanding of the concrete facts of production, distribution, consumption, of the whole economic situation with all its causal processes. To most of this material the processes of specific observation, systematization, and inductive inference are applicable. To much of it, particularly in its dynamic processes, or processes of change, no other method is of any service.

That the great field for research in political economy lies in the analysis of that vast proportion of economic phenomena which are predominantly objective in character, recent tendencies in economic literature and in the scientific activities of economists amply demonstrate. Objective economic phenomena not only can be subjected to specific observations, but can even be submitted to that further process of analysis which consists of the classification, enumeration, and weighing of phenomena. If this is possible, analysis of complex situations into their constituent causes and effects, which Mill maintained was possible only abstractly by deduction, has been made capable of accomplishment by the direct method of induction. Psychological quantities themselves, though not as yet subject to direct measurement, can be measured with some approximation to accuracy through their objective manifestations. Wealth in terms of commodities, for instance, while far from being an accurate measure of welfare, may, for certain problems involving large masses of mankind in varying degrees of prosperity, indicate accurately enough for the purposes of the economist what changes have taken place in conditions of welfare.

Of the methods of induction which we have already described, the method of agreement is always qualitative, and the method of difference when used quantitatively is only rudimentary, and is simply an undeveloped form of a more valuable quantitative method. It was to these two methods in their qualitative forms that Mill and the other writers on economic method have directed most of their attention. This was due in part to their narrow definition of political economy, which would make of it an intensive study of the operations of one psychological motive, and in part to the belief that all the objective phenomena of real importance, at least to the abstract science, were matters of common knowledge and experience, not demanding systematic observation and analysis. If not immediately present to consciousness, the information was obtainable in predigested form from other sciences.

With the accumulation of quantitative data, inference from the facts of common experience and knowledge gives way in importance to the method of inference from facts not discoverable except by methodical and scientific investigation of economic phenomena. Jevons, in arguing against the use of induction in political economy, asserts that the data of the economist are either the results of investigation by other sciences, or else are "old inductions" belonging to the collective experience of mankind.¹ He derives from this contention the conclusion that "specific" or systematic investigation of phenomena is not of value to the economist. But Jevons' facts merely describe political economy as it was when he wrote; they do not detract from the value of the more scientific and productive methods of acquiring data followed by other sciences, unless we grant that political economy has, with such data as it has otherwise secured, already reached a stage of perfection.

The methods of qualitative induction can be applied only to situations where changes in kind have occurred, where a cause and a resultant effect different in kind from those already present are suddenly introduced, or make a sudden appearance in an economic situation. In actual economic situations causes rarely disappear entirely, and as rarely make entirely new appearances. Thus

¹ *Theory of Political Economy*, p. 18.

with the narrow limitations on such experiment as consists in the addition or subtraction of causes in their entirety, the qualitative methods are scarcely ever completely applicable to economic problems, and can be used with any approach to certainty only where the appearances or disappearances, whether natural or contrived, are so great that the original or the persistent elements can be safely disregarded as minor and negligible quantities. While these methods have limited scope in political economy, they are far, however, from being altogether inapplicable. Even those economists who were most decided in their contention that the abstract deductive method was the only one available to the economist made considerable use of these inductive methods in their economic researches. In some cases their chief contributions to political economy were predominantly inductive in character. Thus Mill's entire theory of production, his discussion of the effectiveness of the various systems of land tenure,¹ his analysis of large-scale production and of the laws of increase of labor, capital, and productiveness from land, his explanation of the differences of wages in different employments, his chapter on the future conditions of the working classes, his discussion of the problems of taxation, are characteristically inductive.² Cairnes, who was even more extreme than Mill in his belief in the futility of economic inductions, based his theory of non-competing groups on inductive reasoning,³ supported his theory of international trade by an inductive examination of gold movements and price levels in Australia,⁴ and made an entire volume of an inductive

¹ Based on the analysis made by Richard Jones, an avowedly inductive economist. See Jones, "A Short Tract on Political Economy," in *Literary Remains* (London, 1859), pp. 185 f.

² *Principles of Political Economy, passim*. Cf. Sidgwick, *Principles of Political Economy*, p. 32: "Why then, does Mill say that Political Economy is essentially an abstract science? . . . The only answer I can give is that in this and similar passages Mill is thinking, not of the theory of Production as he himself conceives and expounds it, but of the theory of Distribution and Exchange; and primarily of that portion of this latter subject which he distinguishes as 'statical' and not 'dynamical.' "

³ *Some Leading Principles of Political Economy, passim*.

⁴ *Essays in Political Economy*. ("Essay on the Gold Question.") (London, 1873.)

examination of the economics of slavery.¹ Bagehot was equally inconsistent, since his work was almost entirely inductive—much of it quantitative induction, however—while Jevons made an inductive examination of the coal industry, and also tried to establish, by an application of the inductive method of agreement, the existence of a causal relationship between the recurrence of sun-spots and the recurrence of business crises.

But the quantitative methods give promise of a much wider scope to induction in political economy. Quantitative induction—inference from measured or statistical data—is always preferable to qualitative induction, where the data have been gathered with sufficient accuracy of enumeration and classification, and in sufficient quantity.² The mere fact that the factors in a situation have been measured and classified indicates a high degree of analysis of the situation and of knowledge of its significant elements, and makes it less difficult to avoid overlooking any essential elements.

The quantitative methods of induction are the methods of residues and of concomitant variations, of which the latter will undoubtedly prove to be by far the more important to the economist. The canon of the method of residues is thus stated by Mill: “Subduct from any phenomenon such part as is known by previous *inductions* to be the effect of certain antecedents, and the residue of the phenomenon is the effect of the remaining antecedents.”³ Thus stated, the method assumes that we have already successfully performed several inductions. The method is applicable where a number of causes combine to produce a joint effect, and all the causes but one can be submitted to specific inspection. If we wish to learn how much of the effect is to be attributed to this cause, we can do so by subtracting from the total effect all

¹ *The Slave Power*. (New York, 1862).

² “There is not a clerk nor bookkeeper in the country who is not engaged in recording numerical facts for the economist. . . . It is chiefly a want of method and completeness in this vast mass of information which prevents our employing it in the scientific investigation of the natural laws of Economics.”—Jevons, *Theory of Political Economy*, p. 10. Note the inconsistency of this reasoning with Jevons’ rejection of the inductive method in political economy. See Venn, *Empirical Logic*, p. 437, for a reference to Mill’s inadequate treatment of the quantitative methods.

³ *System of Logic*, I, 437. (Italics mine.)

that is attributable to the other causes.¹ If all the causes are subject to specific inspection, it is a safer and less burdensome method to examine directly the cause whose effect we wish to learn. This method can only disclose a correlation existing at a given moment, and can use statistical material covering a period of time only through separate inferences for each statistical time-unit. The method of residues is, in fact, of value, not so much in demonstrating the existence of a certain causal relationship, as in pointing out an as yet unexplained effect, or portion of an effect, the cause of which is to be sought. The method of concomitant variations, on the other hand, is a direct method of discovering causal relationships where measurement of variations in phenomena over a period of time is possible. It may be stated simply as follows: Wherever some correspondence can be found between the variations of two phenomena in the same situation, we may conclude that these phenomena are causally connected. It is not essential to this method that exact measurement of the phenomena be possible, but only that the intensities, or directions and degrees of change, of the two phenomena shall be comparable. If mortality from consumption varies with the amount of house-room per person, or if output per hour varies with the amount of light in the workshop or the length of the working-day, we may in each case conclude that these phenomena are causally connected.

¹ Mill elsewhere retracts his admission that some of the causes can be measured inductively, and asserts that the method really resolves itself into a series of deductions to discover the effects of a number of causes, in order to infer the effect of the remaining cause. He contends that a direct deduction applied to the cause whose importance we wish to gauge would give us more certain results and would give them sooner.

“Applied to social phenomena the Method of Residues presupposes that the causes from which part of the effect proceeded are already known, and as we have shown that these cannot have been known by specific experience, they must have been learned by deduction from principles of human nature; experience being called in only as a supplementary resource, to determine the causes which produced an unexplained residue. But if the principles of human nature may be had recourse to for the establishment of some political truths, they may for all.”—*System of Logic*, II, 461.

Wrapped up in this reasoning are two further doctrines: first, that even the “disturbing causes” all arise from “principles of human nature,” or are psychological in character; and secondly, that even in the measurement of disturbing causes, or what Mill calls the “application” of political economy, induction is not an available method.

Where several causes combine to produce a given effect, the effect may not vary with variations in one of the causes if these are offset by variations in the remainder of the causal situation. The method is applicable with a great deal of certainty where one element in the situation varies greatly, while the remainder of the situation is fairly constant. Under such circumstances, which among economic phenomena are by no means rare, the minor variations may be disregarded, especially since exact results are not required in political economy.

Even where several of the concurrent causes vary considerably, the method is often applicable, although with a lesser degree of certainty. Artificial elimination of the disturbing cause may be possible, although this resource is of course very limited to the economist. Or it may be possible to discover the amount of influence exercised by the other factors by their examination in other fields. If the variations in the other factors follow a regular ascertainable order of change, the comparative importance of these factors at the different moments of time can be gauged, and allowance made with some accuracy for the amount of change in the effect to be attributed to the variations in these factors. If a great number of situations be studied, and most of the causes combining to produce the effect are minor ones, it will often be found that the changes in these minor causes tend to neutralize each other. Since in all these cases precise results are not demanded, the permissible degree of error may be considerable, without destroying the value of the results if the variations in the essential factors have been great.¹

For preliminary induction by the method of concomitant variations, it is sufficient to know that A varies as B, without further knowledge of the data which these symbols represent, to infer a causal relationship between A and B. It is on the basis of preliminary induction that many of the statistical tests of correlation and methods of allowance for error find their logical justification. But for true induction, the fact that a correlationship of

¹ Cf. Jevons, *Principles of Science*, pp. 340-46, for a discussion of these methods of correction of results as applied to the method of difference—of which, of course, the method of concomitant variations is a special form.

phenomena is found, and persists throughout the period of investigation and throughout all the situations studied, would not be accepted even in the absence of contrary evidence or of the expectation of the discovery of contrary evidence, as a sufficient demonstration of the necessity of such a concurrence. For true induction, there is further demanded a direct acquaintance with the data and the situation, and an intuitive belief arising from the insight obtained through such acquaintance, that in each instance of the concurrence, such concurrence seemed a necessity of the situation, and not a possible coincidence. In concluding from an observed concurrence between two statistically measured phenomena that the concurrence is a necessary one, our inference obtains greater scientific validity when we discover that the phenomena we knew as A and B are congestion and mortality, or fatigue and output, or similar phenomena which by their known characteristics suggest the possibility of relationship.

Correlations in statistical data do not demonstrate themselves automatically. Some hypothesis is a preliminary to every inductive investigation. What elements of the situation shall be measured, and which of the measured elements are to be compared in the search for causal relationship, cannot be determined by any set rules. Discovery, whether direct discovery of the relationship within a mass of accidentally contiguous data, or, what is more probable, indirectly through the flash of insight which suggests a possible hypothesis, confirmation of which is to be sought in the data, is an essential preliminary of the inductive method; and discovery, in all its phases, is never automatic, or the simple result of formulated rules of discovery mechanically followed.¹ But direct knowledge of, and keen insight into, the situation to be analyzed is necessary, not only as a preliminary to the discovery of causal relationships between constituent elements, but for the knowledge of what are its constituent elements.

The method of concomitant variations involves the resort to a number of assumptions, in the sense of propositions incapable of

¹ See Jevons, *Pure Logic*, p. 295, for a criticism of Mill's failure to emphasize this in his exposition of the methods of induction.

complete proof, which may detract considerably from its reliability, especially where the inference from the mere uniformity of recurrence of the phenomena is not supported by the intuitive belief of the necessity of the concurrence in each individual instance of its appearance. The assumption that we can reason from a number of instances of causal relationship to the universality of such relationship is of course the fundamental assumption of induction, which is itself derived from the fundamental assumption of all processes of reasoning that there is a uniformity of causation in nature. We have both instinctively and reflectively sufficient confidence in such uniformity to make such assumptions justifiable, until evidence to the contrary is discovered. The assumption that A is a cause of B because it varies with B is not justifiable, however, without further evidence. A and B may vary concurrently, not because A is a cause of B, but because B is a cause of A, or both are effects, or different aspects of the effect, of C. Day follows night, not because night is a cause of day, but because the day-night sequence is the result of a third factor, the revolution of the earth. Generally, however, we are not confined to the knowledge of a mere concurrence, for (1) we can either intuitively "see" that A causes B in these instances by actual inspection of the phenomena; or (2) we may find some intermediate step in the causal process between A and B which explains the sequence; or (3) we may find corroboration in another inductive inference, or in a deduction, for the conclusion that A is a cause of B; or (4) we may find that a variation in A always precedes a variation in B, thus indicating A-B to be the direction of the causal sequence; or (5) we may exhaust the causes present in the given situation, and find that no cause other than A is competent to cause the variations in B; or (6) we may be able to demonstrate, experimentally or otherwise, that the causes operating on the one do not come into contact with the other, and therefore cannot be the proximate causes of both; or (7) we may find that A and B are really different quantitative phases of the same phenomenon at different moments of time, and may thus be led to seek some element in the causal situation which is present when the one phase appears, and absent when the other is present.

It is partly owing to these considerations that statistical data gathered at too long intervals, or with too long a period of time taken as the statistical time-unit, are often of little value to the student. Yearly price-statistics, for instance, would conceal the influence of the autumn demand for media of exchange on the autumn price-level, as would yearly output statistics conceal the effect of different weather conditions during the different seasons on the efficiency of labor. The lower price and the higher output in the autumn—if such happens to be the case—as compared with the situation in summer, would suggest the search for some elements in the causal situation not present, or not present to the same degree, during both seasons. But if any of the above tests could be applied, it would demonstrate that the high price-level of the summer was not the cause of the lower autumn price-level, and similarly that the high output in the autumn did not result from the low output in summer.

The methods of induction and of deduction are coessential to any science dealing with concrete facts. All knowledge rests ultimately either on inductive premises, or on hypotheses, which, if modified to fit the facts, become inductive in character. Deduction finds its scope in the application of inductive generalizations to particular instances or lesser groups of instances. There is a further field which is purely deductive, if it be proper usage so to term the derivation of generalizations from the facts of introspection. An inductive generalization covering a given class of instances may be verified by a deduction from a wider generalization applying to a class of instances of which the given class is only part. The premises of a deductive inference may be verified by specific inspection of some of the instances covered by these premises. A method may be used for purposes of exposition and persuasion which differs from the method of discovery, but corroborates its results. There is but limited scope for experiment granted to the economist, and what experiment does take place is rarely under the control of the technical economist, and never accompanied by the degree of control of the material circumstances which the physicist considers essential to proper experimentation. The use of carefully enumerated and classified data,

covering a great number of phenomena, is the resource available to the economist as a substitute for experiment. The economist uses all these methods. He cannot confine himself to any one method alone, if his science is to deal with actual facts, since the method used to acquire knowledge is different from the methods used to verify and to apply it. The data of the economist are not hypothetical, nor are they entirely, or even predominantly, obtained by introspection or borrowed from other sciences. The larger part of economic data is to be derived from an inductive study of the industrial organization of society and of the behavior of mankind in industrial society. But the two methods of obtaining data are of co-ordinate importance. Neither is precluded to the economist, and neither serves merely as a nonessential subsidiary to the other. The difficulties of technique of both methods are equally great, and knowledge of both is demanded of the economist.

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